

clusions on the subject, that "berries obnoxious to birds will stand a better chance of propagating and increasing that variety?" If C. W. W. (NATURE, July 7) will turn to Letter 55 of White's "Selborne" he will find the following observation on the House Martin:—"The young of this species do not quit their abodes all together, but the more forward birds get abroad some days before the rest. These approaching the eaves of buildings and playing out before them make people think that several old ones attend one nest."

Our Middle-Class Schools

I WISH to bring before your readers the necessity of immediate action with regard to a branch of education at present not liable to legislative interference. Government is becoming more and more alive to the fact that Education and Science at the present are England's greatest needs; hence the steps taken to extend and enforce primary education. But whilst increased facilities are being afforded to raise the standard of primary education, secondary education is at a stand-still, and upon the whole falls far short of the point it should reach. Thousands of our middle-class schools when compared with what is required, may be placed in the same category as the old dame's school when compared with the modern national school. It requires but the slightest knowledge of the subject to know that our middle-class educational system is as a whole a mere farce, and yet so averse are we to change, that matters are allowed to go on year after year in the same old matter-of-course style without the slightest indication of reform. In order to encroach upon your space as little as possible, I will in a succinct and concise form lay before your readers a scheme which has been lately mooted, which has received the sanction of the highest authorities in these matters, and which is destined ultimately to bring about quite a new system.* In speaking thus indiscriminately of our middle-class schools, I do not include many excellent institutions, in which a thorough course of training forms the routine, and which are conducted by gentlemen capable and willing to do the work required. Alas that there should be so few!

Then 1st. It is well known that individual influence is of little service. This fact supports the theory that an association must be formed, consisting of the principals and assistants of middle-class schools, and others interested in the question.

2nd. This society should have certain objects, and its members combined should use their utmost endeavours to assist in carrying out these objects. A few of the aims would be as follows:—

a. The institution of normal colleges for the training of gentlemen who wish to enter the scholastic profession.

b. To recognise some examination, diploma, &c., as sufficient guarantee of the capabilities of gentlemen entering the profession, and insist that such gentlemen shall have this diploma. The evils arising from the incapability of so many of our masters cannot be over-estimated.

γ. The necessity of Government or other central supervision and examination of every school. At the present moment the standard of a school is calculated by nothing. An advertisement perhaps appears, stating that all boys sent to special examinations have passed; and instances are known where one boy has been sent up to such examination. It is impossible to decide upon the general tone of a school by the examination of a few of the best boys.

δ. The institution of a club-house in London where appointments could be made, business transacted, &c., and attached to it some means by which the incubus of agents could be avoided.

ε. Periodical meetings, &c., &c.

I am afraid this letter is running to an inordinate length, but I just wish to add that invitations have been issued by the editor of the *Quarterly Journal of Education* to a few representative gentlemen for a private preliminary meeting to be held in September next, when the above scheme is to be discussed. Any gentleman wishing to take part in that meeting should address the editor upon the subject. I might have referred to the failure of the College of Preceptors to do the least good. What we must have is an obligatory examination of the whole school, and every school; not leaving it to the whim of the principals. Neither are assistant masters treated as they should be by the College of Preceptors.

X

The Source of Solar Energy

MR. GREG ascribes to me views I do not hold, and then employs my own reasoning to overthrow them. He must have formed his conceptions of my theories from Prof. Pritchard's critique of my "Other Worlds"—a most unreliable source.

To begin with,—I do not believe that the solar heat supply is solely derived from the downfall of meteors. I have impressed this very clearly at p. 54 of my "Other Worlds."

I do not believe that any part whatever of the solar heat supply is derived from meteoric percussion, nor that any meteor ever comes within tens of thousands of miles of the sun's surface in the solid state.

Mr. Greg is very careful to show me that the meteor-systems encountered by the earth cannot fall into the sun. I dwell on this very fact at p. 203 of "Other Worlds"—I say, *totidem verbis*, that no known meteoric system can form a hail of meteors upon the sun. "It is forgotten," says Mr. Greg, "that the meteors themselves revolve round the sun," &c. If he has at any time forgotten this, I certainly have not.

"Has it ever been proved," he asks me, "that the entire mass of meteors constituting the zodiacal light, is either composed of matter in a solid state, or, if it were, that its mass would be equal to that of our own earth?" I answer, as Mr. Greg would—"No, it has not been proved, nor is it by any means probable."

There is nothing new to me in Mr. Greg's letter, and little which I have not described myself long ago in the *Intellectual Observer and Student* of 1867, 1868, and 1869. To suppose that I should venture to treat at all of meteoric astronomy, in ignorance of such elementary facts—the very A B C of the science—is not complimentary. Mr. Greg might, at least, have examined what I have written before assigning to me the absurdities he attacks so successfully.

The fact is, this matter of the solar energy only comes in *par parenthèse* in my "Other Worlds." I express no confident opinion whatever about it. I point to some deductions from known facts, and respecting them express a certain feeling of confidence. It is not my fault (nor, indeed, can I blame Mr. Greg) if Prof. Pritchard has tacked my words "I am certain" (used with reference to reliable inferences) to a theory respecting which I have distinctly written, that "I should not care positively to assert" its truth. Even that theory is not the absurd one attacked (very properly) by Mr. Greg.

For the rest, most of Mr. Greg's letter is sufficiently accurate, but there are two mistakes in it.

1. We have abundant evidence that the density of the aggregation of cometic perihelia increases rapidly near the sun. For example, whereas between limits of distance 40,000,000 and 60,000,000 miles from the sun this density is represented by the number 1.06, it is represented by the number 1.67 for limits 20,000,000 and 40,000,000 miles, and by the number 8.65 within the distance 20,000,000 miles. The evidence derived from this observed increase of aggregation is not affected by what we know of those cometic or meteoric systems whose orbits nearly intersect the earth's (for they must form but the minutest fraction of the total number) nor by the observed minimum perihelion distance of cometic orbits (for observed comets are but the minutest fraction of the total number).

2. It makes no difference whatever as regards the force-supply of the solar system, whether the substance of a meteor reaches the sun in the solid, fluid, or vaporous state. Given that the substance of a meteor, moving at one time with a certain velocity at a certain distance from the sun, is at another time (after whatever processes) brought to rest upon or within the sun's substance, then either the "force-equivalent" of its motion has been already distributed or the substance of the meteor is in a condition to distribute that "force-equivalent" mediately or directly. In other words, either heat and light have been already distributed, or the central energy has been recruited to the full extent corresponding to the mass, motion, and original distance of the meteor.

I may express here my agreement with the opinion of the Editor of NATURE that the observations made on the zodiacal light by Lieut. Jones and M. Liais ought to be taken into account in any theory of that mysterious object. Taken in conjunction with the other known phenomena of the zodiacal light, they admit of but one interpretation as to the position, dimensions, and general characteristics of the object. Taken alone, we might infer from them that the zodiacal light is a ring of bodies or vapours travelling around the earth (at a considerable distance);

other phenomena suggest that the zodiacal light is a disc of bodies or vapours travelling around the sun; yet others suggest that the zodiacal light is a phenomenon of our own atmosphere. But the only theory which accounts at once for all observed phenomena, is that which regards the zodiacal light as simply due to the continual presence in the sun's neighbourhood of bodies or vapours (meteoric or cometic, or both) which come there from very far beyond the earth's orbit, and pass away again on their eccentric orbits. A disc thus formed of continually varying constituents would shift in position, and would wax and wane in extent as well as splendour, precisely as the zodiacal light is observed to do.

RICHARD A. PROCTOR

Spontaneous Generation

THE physical capacity of fungus-spores to throw out mycelium, and from that to be able to reproduce a parent (or, according to Dr. Bastian, to produce a fungus *de novo*), shows a complicated organisation greatly above that of the monad. From a careful examination of Dr. Bastian's experiments and figures, I am led to believe that the majority of the ovoid bodies referred by him to fungus-spores are nothing of the kind, and that if they really belong to the vegetable kingdom at all, they are perfect unicellular plants in themselves reproducing their kind by subdivision; the presumed mycelium I should refer to the bursting of the cell-walls, and consequent discharge of the contents, a by no means uncommon occurrence with unicellular organisms. It is, however, impossible to follow the author in his speculations regarding these bodies, as his measurements are so imperfect, and in several instances, where most wanted, omitted altogether. A few of the bodies certainly bear an external appearance to fungus-spores (for instance C, Fig. 11, which might be referred to *Russula* or *Scleroderma*), but as no size is given it is impossible to form an opinion. Perhaps Dr. Bastian will say on what data he refers such objects as are shown on Fig. 3, to fungus-spores, and by what characters he knows the mycelial filaments to be such: the "half-grown" spore (?) described on page 197 has most extraordinary characters for such an object; for, says the author, "the nuclear particle within was seen moving from end to end of the cell."

Had not Dr. Bastian distinctly affirmed that the spores were generated at once from heterogenous materials, I should have assumed his belief in the presence of the perfect plants in the infusions, though the detached spores were all he met with in his experiments; for if any organism originates independently of a parent similar to itself, surely it is reasonable to expect the production to be at once perfect, and not in the egg state. Could an oviparous animal be produced from heterogenous materials, surely one would not expect eggs first to appear. In referring these unicellular bodies to spores, Dr. Bastian appears to me to have defeated the very object he had in view.

That some of the bodies figured are *bona fide* spores of fungi seems very probable, and that they were produced by ordinary parents, seems equally so, for the slight neck-like elongation or spot, which is analogous with the placental scar in animals and flowering plants, is clearly present in the lower right-hand object (Fig. 11) and the lower left-hand object (Fig. 13). If these things were evolved without parents, surely nature would not have given them an umbilicus; it is far more reasonable to suppose that true spores got into the infusions (and perhaps germinated) by some accident similar to the three recorded instances where foreign bodies were undoubtedly found. (Note, p. 197.)

It seems to me rational enough to suppose that unicellular bodies and objects of the lowest possible organisation may be heterogeneously produced from the inorganic world, for here the line between one and the other is so fine as scarcely, if at all, to be perceived; indeed, the Brownian motion of monads, some spermatozooids, and the particles forming many inorganic infusions, are scarcely, if at all, to be distinguished from each other. From monads and unicellular organisms, however, spore-producing fungi are greatly removed, and the economy, functions, and structure of most of the latter are now so well known, that it would be simply impossible to convince any botanist that a spore like C (Fig. 11) could be produced from any other quarter than the hymenium of a well-defined parent.

I cannot see that the production of motile zoospores in Achlya (p. 174) has any bearing on the subject, as here we have an already living parent; and motile zoospores are by no means uncommon in the vegetable kingdom; their movements, how-

ever, so far as I have been able to observe them, do not differ from the Brownian movements seen in the inorganic world.

It is clear that no definite conclusions can be arrived at regarding these bodies and their production till a series of accurate figures is published to a uniform scale (with an indication of the colour of the cells, &c.), enlarged at least 2,000 diameters, for even then a monad would be no larger than a pin's-head. It will then be possible to compare the bodies with actual fungus-spores and other bodies well known to botanists and zoologists.

Prof. Wanklyn appears to be unable to estimate the number of germs of fungi known to exist in the atmosphere. That "there must be very many of them" is apparent from the calculation that, if each spore of one species only of one of the higher fungi germinated and reproduced its parent, the children would, in the first generation and in the course of a very few days, form a carpet all over the earth. Now, as fungi abound everywhere in myriads, and the family is almost illimitable, the number of diffused germs is evidently beyond all calculation; their size, too, is often so small that twenty could be conveniently accommodated on the diameter of a single human blood corpuscle. That they are alive is proved by the readiness with which, under favourable conditions, they may be made to germinate. It must also be admitted, in Prof. Wanklyn's favour, that "they must weigh something," though I am not aware of any attempt in that direction at present. Some of their "remarkably small" component parts are, however, made manifest by chemical reaction.

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Super-Saturation

THE following experiments may be found interesting from their bearing on the latest theories advanced on the subjects of super-saturation and the so-called inactive state of bodies. Professor Tomlinson's theory is that a super-saturated solution adheres as a whole to a chemically clean surface, but that a differential adhesion takes place in presence of a chemically unclean surface, because the salt or gas adheres to such a surface while the liquid does not; the former is consequently liberated. The presence or absence of grease is then stated to constitute chemical uncleanness or cleanness. If a greasy surface can be rendered inactive, it is clear that both these propositions cannot be true; either grease is not of itself a cause of uncleanness, or unclean surfaces are not necessarily active ones. The following experiments prove that the fats may be rendered inactive by the same processes which are applied to rods of glass or metal.

1. A bit of composite candle was melted with a very little alcohol; a glass rod was dipped in, allowed to cool, passed through flame of a Bunsen's burner, and a drop of melted fat deposited on surface of supersaturated solution of zinc sulphate. The fat solidified without affecting the solution even on prolonged agitation. A crystal of the salt caused instant solidification.
2. Some solution boiled remained supersaturated for a fortnight with a crust of fat on the top.

3. Ordinary tallow treated as No. 1. Inactive in solution of sodic sulphate solution; touched with finger, crystallised at once.

4. Lard cleaned simply by melting on rod passed through flame, allowed to cool, stirred in solution of sodic sulphate. Inactive. Other end of rod active at once.

5. Same solution boiled; cooled, stirred with rod, treated as the last, and left exposed to the air for 15 minutes. Did not crystallise. This experiment is interesting, as showing that greasy substances are not specially liable to be made active by exposure to ordinary air.

6. Some tallow was melted in a test-tube without precautions of any kind, and while melted was poured to the depth of half an inch on to solutions of ferrous, cupric, and sodic sulphates. In each case it was inactive. This is conclusive on the point in question.

Theoretical objections have been urged against the definition of chemical uncleanness, and I think this might now be surrendered.

Professor Tomlinson may very likely be right in looking to adhesion for an explanation of these phenomena; at all events, far greater probability seems to attach to this view than to that put forth by M. Gernez, adopted by Jamin, and even, I believe, favourably noticed by the Academy itself; which is that only a crystal of the same salt can induce crystallisation. This latter view is open to the theoretical objection that it necessitates our believing that all salts capable of supersaturation